leave only gas as interstitial material. This is, however, not the only possibility. It is not difficult to conceive of a distribution of forces in the heterogeneous surroundings of an igneous mass such that locally the liquid might be sucked out of a crystalline mesh, which would acquire a miarolitic texture, temporarily, at least. The consequences of this action would normally differ in no essential particular from those produced by simple squeezing out of the liquid. It is probable, however, that this sucking action could be operative at a very late stage of crystallization when squeezing out of liquid may be impossible or at least very unlikely. The kinds of liquid that form some of the alkaline rocks may perhaps be removed in this manner from quartz-mica rocks at a very late stage of their crystallization and possibly only such action can effect the separation of these liquids.[1] In certain regions of the earth's crust where tangential extension is the dominant expression of the forces acting (Atlantic structures) the development of alkaline rocks might be a prominent feature though the conditions requisite to their formation would undoubtedly occur locally elsewhere.

[1] The Later Stages of the Evolution of the Igneous Rocks, *J. Geol.*, **23**, 1915, Suppl., pp. 1–91.

[2] Howie, Robt., Summary *Rept. Geol. Survey Can.*, 1916, Coleraine Map Sheet insert, p. 228; also Knox, G. K., *Ibid.*, p. 229, et. seq. Reference is made to granitic "batholiths" in the anticlines, p. 232.

THE EVIDENCE FOR THE LINEAR ORDER OF THE GENES

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Despite Castle's dictum that we "have failed in two different attempts to establish the linear theory in the case of the three genes yellow, white and bifid," we are bold enough to maintain that the data furnished, and still furnish, the proof called for. We wish to call attention to the fact that in his last paper Castle ignores our proof of the linear order that is furnished by building up the whole chromosome (or even large sections of it) by "distances" so short that no double cross-over classes appear.

Castle asserts that we have rejected "nearly 99 per cent" of our data in the construction of the yellow, white, bifid section of the map. As a matter of fact no data have been omitted. In this case, as always, the order of the loci was determined by experiments that involved all of these loci at once. The order having been established the next step was to determine the relative distance between the loci by the use of all the available data. We have emphasized in our reply to Castle that there are several sources of variability in linkage values such as age, temperature, genetic factors. The variability due to these causes far outweighs that due to random sampling. It is, therefore, inadmissible to compare data from different

experiments, however extensive, in establishing the order of the loci, or in testing the validity of the hypothesis of linear arrangement. For such purposes, as we have already pointed out, it is essential to use data in which all the loci are followed at once.

The purpose of the maps is twofold: first, to give the sequence of the loci, and secondly, to indicate, by the relative spacing of the loci, the cross-over values most likely to coincide with the results of future experiments. For the latter purpose it is evident that mean values are needed. These can best be obtained by using all the data. And this, as stated, has been the method used for determining the distances on the maps published. Castle has used all the data for criticizing the sequence of the loci on our maps, and has accused us of using only the three-point data for determining the map-distances. Obviously this is an inversion of the correct relationship.

Even when all the available data are used in constructing the map, these data should themselves be subjected to all the controls that it is possible to apply to them, as everyone familiar with the treatment of rough data will understand. For example: It is well known that under certain conditions, such as crowding, some mutant classes run behind their expected ratios. Under favorable conditions of culture, normal ratios are obtained. Clearly, data of the first kind unless corrected are unsuited for determining distances on the chromosome map; and the larger the amount of such data, the larger would be the discrepancies in a map based upon such data used with normal data. We have shown how the aberrant ratios due to the inviability of given classes can be balanced by the use of converse crosses. Further corrections and weighings are also desirable in order that no one class of data shall unduly prejudice the result. The methods for making such corrections have been briefly outlined in Carnegie Publication No. 237, 1916, and given more fully in Carnegie Publication No. 278, 1919.

Besides environmental disturbances of the kind just described there are factors that are known to modify crossing-over. Obviously such data should be eliminated from material from which a normal chromosome map is to be made. That Castle himself admits the validity of elimination of such data is shown by the fact that he deliberately rejected the data involving lethal 2 which we had shown to cause aberrant linkage relations. If he had used these data, all of his long wires would have been bent. Castle has set up the claim that one of the advantages of his model is that such a bending of a wire in the case of white forked revealed the fact that the experimental value of this interval was too great. The same fact would have been "revealed" on any system by a comparison of the white forked value with the values for white rudimentary and white bar, both of which were based on larger numbers than white forked itself.

Castle's discussion of interference in connection with yellow white bifid (p. 503) shows his failure to understand interference, and hence his criti-

cism misses the mark. It need only be stated that Castle was wrong in supposing that the cross-overs between yellow and white should be subtracted from the total before the white bifid cross-over value is calculated.

If, as Castle states, he cannot conceive of a mechanism "which would tie two genes together in such a way that they will subsequently separate from each other oftener than they will remain together, yet this is what the idea of cross-overs in excess of 50 per cent amounts to," it would seem to follow that he has not really understood the mechanism that we have described, and which he has attacked; for, whether such a mechanism really exists or not, it is nevertheless a conceivable mechanical device that could do just this thing.

We have left to Dr. Metz the opportunity to answer Castle's criticism relating to D. virilis.

To sum up: we believe that we have met all the pertinent criticisms that Castle has brought forward of our methods and conclusions, and that he has failed to meet our criticism of his three dimensional model.

THE ARRANGEMENT OF GENES IN DROSOPHILA VIRILIS

By Charles W. Metz

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Communicated by T. H. Morgan, March 6, 1920

In connection with his general attack on the theory of the linear arrangement of genes Castle[1] has questioned my use of this theory in explaining the genetical results obtained in Drosophila virilis. I did not consider it necessary to reply to this criticism because Castle's general position was shown by Sturtevant, Bridges and Morgan[2] to be untenable as regards Drosophila melanogaster, with which D. virilis agrees in its mode of inheritance. Subsequently, however, Castle has reaffirmed his belief in the superiority of his hypothesis over that of linear arrangement[3] and has apparently misconstrued the silence on my part with respect to Drosophila virilis. This would seem to call for a brief reply.

From my paper on eight sex-linked characters in D. virilis[4] Castle concluded that the genes dealt with could not be arranged in a linear series, and by applying his three dimensional hypothesis to the case he made certain predictions (*b*, p. 36) which he now wishes used as a test of his hypothesis. His general arguments in this case are the same as those he used previously in reference to Drosophila melanogaster, and since they are being treated by those toward whom they were first directed, I will avoid repetition by confining myself to his predictions. These apply to four undetermined cross-over values in D. virilis, namely, magenta-hairy, glazed-rugose, frayed-forked and frayed-glazed.

It should be pointed out first that in making these predictions Castle apparently overlooked the statement in my paper (pp. 113 and 125) that the frayed stock had been lost shortly after it was obtained. The absence